Narratives of enterprise revisited

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Narratives of enterprise revisited: methodological appendices in ethnographic books

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Abstract

Purpose – This paper is the first in a series that reprints methodological appendices or methods chapters found in workplace and organisational ethnographic books, and provides an opportunity for reflection by the author through an introductory commentary. Simon Down, the author of Narratives of Enterprise (Down, 2006) reflects on the writing and the research underpinning his ethnography. The paper aims to discuss these issues.
Design/methodology/approach – The reprinting of such chapters will enhance access to key ethnographic texts, and facilitate reflection on methodological choices authors made. In so doing this paper will provide insights into methodological ethnographic writing, and show how sensibilities and fashions change over time.
Findings – Narratives of Enterprise (Down, 2006) examined how two small business managers in a single firm construct an entrepreneurial self-identity, and what this process of self-creation means for the individuals and how the firm is managed. The key topics explored in the book, self-identity as a conceptual tool and enterprise as a social and economic reality, have both grown in relevance and importance since the research was conducted. Down also reflects on that nature and dynamism of friendship in research practice.
Originality/value – Reflection on choices made at some distance can provide particular and valuable insights into the development of research practice.
Keywords Narratives, Entrepreneurship, Ethnographic books, Friendship and research practice, Methodological appendices/chapters, Self-identity
Paper type Research paper

Introduction

Authors of ethnographic books often place the details of how the research was undertaken in methodological appendices. They are not essential for the completeness of the work. Not an afterthought but placed as an accompaniment, of additional interest to fellow ethnographers, less so to readers. Methodological matters matter to professional researchers. An appendix normally means the book is a revised thesis too. Perhaps it is better to leave them out, or weave the important legitimating and framing details into the book's beginnings? An appendix is a compromise.

For professional researchers these chapters offer further credentials and give processed insights to neophytes engaged in their own PhD thesis-based ethnographies. Processed because honesty and sincerity are worn like a wedding suit: only the most interesting, most buttressing and most humanising details are deployed. Methodological tales are no less crafted than others.

I was clear that my methodological chapter would be appended in the book of my thesis. In the PhD (Down, 2002) this chapter was chock-full of insurance passages designed to assuage a wisely cautious supervisor. Lengthy defences and explanations of ethnography and its philosophical claims would not be necessary in the book (Down, 2006). Of course the main function of an appendix is to build authenticity, but for me it was more of a stylistic decision: it seemed part of the shape and tradition of such books.

Narratives of Enterprise: Crafting Entrepreneurial Self-identity in a Small Firm (Down, 2006) appeared four years after the thesis was submitted, examined (by Tony Watson and David Goss) and awarded. This might seem a long time but is easily filled by book proposals, rejections from publishers, dealing with proposal reviews, re-writing and the production process itself. When I completed my thesis I thought of the work as contributing to organisation studies more than entrepreneurship. Indeed I felt, and still feel, a greater affinity to organisation and small business studies than entrepreneurship. Yet, my contribution, such that it is, has been one of many in recent years that have sought to humanise the actions of people who run small businesses. The concept of self-identity, which when I began the empirical work in 1997 was quite novel in organisation studies and almost non-existent in entrepreneurship or small business studies (Fournier and Lightfoot, 1997, were an exception, and see Down and Giazitzoglu, 2014 for a recent review of this literature), has since become very popular in many fields.

Sociological theories of self-identity implicitly challenge the view of entrepreneurship implied by most other economic and psychological perspectives. Rather than an exceptional and heroic activity identity aware research has shown that like everyone else people engaged in entrepreneurial activity craft narratives in mundane, everyday ways, grabbing whatever discursive and material resources suit their current purposes. As I wrote then:

> Overall this study suggests that we should talk of entrepreneurialism and enterprise in quieter voices. In our rush to heap praise on the impossibly heroic entrepreneurial protagonists that seek out the holy grail of enterprise generated prosperity, or alternatively condemn the pursuit and proliferation of “enterprise” as some monstrous destroyer of civic cohesion, we have lost sight of the durable everyday nature of this activity. It turns out that rather than being superhuman Paul and John are just ordinary folk: this book shows how enterprising activity and the narratives that support it create ordinary, believable, everyday entrepreneurial selves (Down, 2006, p. 12).

The story my ethnography tells about the two business managers is, upon some further reflection, about their careers. Though they were running a small, growing business during the two and a half years I was doing the research, they had been employees earlier in their careers and would become employees again a little after. Later still they would set up their own business again. We see them doing entrepreneurship (or entrepreneuring, Steyaert, 2007) rather than being entrepreneurs. Entrepreneurship discourses are portable and disposable.

In addition to the attractiveness and potential of applying theoretical ideas around self-identity to entrepreneurship, the study also sought to provide some “bodied stuff” (Geertz, 1973, p. 23) to often speculative debates on neo-liberalism, increasing social atomisation and the rise of an enterprise society. I was interested in the consequences of individual identifications with enterprise discourses. And, although the plasticity of self-identification implied that retrieval, repair and reversal were all achievable, and that identification with entrepreneurship was mobilising and empowering, the study also demonstrates that the identity making of the two business managers tended "to reflect some of the atomistic and narcissistic coping strategies of modern life" (Down, 2006, p. 113). Paul and John created an "over-protective cocoon" in order to "provide them with the 'fixity' necessary for taking the risk inherent in their venture" (Down, 2006, p. 111). Since then economic instabilities, declining living standards in the developed world and increasing employment precariousness have been exacerbated by financial crises. Hopes of a collective reassessment of neo-liberalism have proven ill founded. Entrepreneurially based public narratives and discourses have become more popular with many, as opportunities for traditional career employment have declined or become less financially rewarding for greater numbers of people. For many starting a business is seen as an escape or a lifeline. As Down and Giazitzoglu (2014, pp. 109-110) put it:

[…] so attractive has [being an entrepreneur] become for some people that an attachment to actually running a business is only a tenuous requirement. In the absence of more secure forms of work some people (particularly the young) now cobble together a mix of internships, volunteering, social enterprise and business preparation and opportunity assessing activity (often assisted and validated by government agencies, universities or corporate business competitions) and call themselves "entrepreneurs" in the way others might call themselves "actors" or "consultants".

Both self-identity as a conceptual tool, and enterprise as a social and economic reality have grown in relevance and importance. As is typical with ethnography, the focus on the particular has not meant that the insights generated by narratives have become redundant with the passage of time. Together with other enterprise-focused ethnographic books by Hobbs (1988), Kondo (1990), Ram (1994) and Holliday (1995) the insights often achieve a form of temporal suspension. Clearly a conceit in some ways, achieved through craft, nevertheless durability is a curious feature of a preoccupation on the particular.

As for the durability of how the research was conducted, the methodological choices elaborated in the chapter below still seem to debate relevant and interesting issues. What, if I reflect on the reflection the appendix represents, do I now think of the choices I made?

What should be immediately apparent is the tension expressed between pragmatic and dogmatic varieties of ethnographic (or social scientific) practice. I remain attached to the pragmatics "scrum" (Down, 2002, p. 97). And two deeply subversive influences swirled diaphanously through the development of my ethnography. Feyerabend's (1975) book *Against Method* and Murray S. Davis' hilarious pomposity-busting 1971 essay on the philosophical and rhetorical conceits that underpin the social scientific enterprise, both rather undermined my confidence in the social scientific, whilst simultaneously strengthening my belief in the practical and intellectual power of well told stories about the social.

I am older and more experienced now. I am still deeply sceptical and suspicious of those methodological dogmatists that stipulate exacting methodological rules and paint-by-numbers detachment as the only route to social scientific insights. However, greater experience and tolerance of the practices of other social scientists has shown that differences between pragmatists and dogmatists, often amount to preferences about how many notes to leave in the margins. Both can produce powerful research that shapes reality. Ethnography's power is derived from its authenticity-creating marginalia, whereas studies based on methodological dogmatism (whether qualitative or based on number and quantification) tend to seek elegance and insight from abstraction. Deep down, both are but rhetorical choices.

A key reflection in the chapter below is how my personal experiences gained greater coherence, both conceptual and narrative, through the production of a PhD and a book. The process of writing often does the thinking for us (Down, 2001, pp. 1647-1648), and I expressed a degree of disquiet about the tidier version of events my writing had crafted. As time has passed and actual memories of fieldwork have been progressively replaced by an amalgam of memory, written artefact and their use by others (i.e. citations), this concern has dissipated. Originally I rightly felt concerned that the Paul and John I had created were not as real as they were in life. The concern I raised was in all honesty excised by the asking; it is a naïve question. I am more equivocal now about our ability to depict a given reality. Paul and John were my creation: their approximation to real events is limited in many ways and senses. My regrets now reflect what all writers, social scientist or not, lament: I wish they were better written. But since my memory or indeed the original data, particularly the interview tapes and transcripts, which seem quite without personal resonance now since they are dominated by the versions I have created, cannot provide a realer alternative, as I clearly thought it did at the time the chapter was written, the PhD, book and articles are all we have.

Another concern that peppers the account below is the role of friendship in research, particularly reflecting on my relationship with Paul. Our friendship, which pre-dated the research, and the break up of his marriage, which began towards the end of the fieldwork, was an important sub-context for the whole project. Clearly being friends with Paul enabled the research. However, the analysis about friendship I applied to Paul and John's relationship is equally valid for my own with Paul. In Chapter 3 (Down, 2006, p. 43) I used Sennett's (1981, p. 4) observation that friendship is a bond in both the sense of a connection and a constraint. This was also true of my own friendship with Paul. The connection clearly facilitated the research, providing access to the firm and ease and speed in building rapport with Paul, and, by association and shared trust, to others in the firm. However, my bond to Paul perhaps constrained aspects of the research as well. Would the others in the firm have been even more open with me had Paul simply been a professionally interested but detached research gatekeeper, rather than a friend? Did I sub-consciously self-censor in some ways, on some topics, because of my friendship and loyalties to Paul. Did I sub-consciously soft-peddle, or paradoxically perhaps even over-do, my critical analysis because of the constraint of fraternal ties? Undoubtedly, as I speculate in the appendix, things would have been different.

Although for me the nature of our friendship changed for the worse as a result of the proximity afforded by the research, nothing especially dramatic happened. There was no dramatic break up or argument at the end of the research. Perhaps much was left unsaid, as is the way of these things. It was easy to let our friendship drift. I lived in Plymouth, then Australia, and just was not in Maltonbury very much now that I had less reason to be there.

Today, I do very occasionally still see and e-mail Paul, since other mutual friendships and family connections ensure that ties will never be totally severed. However, lack of proximity and the passing of time has meant that we do not make an effort to keep each other up to date with our lives. We are acquaintances.

Part of what happened during the research, I think, was that in being a researcher I became more subordinate to Paul. It was Paul that led the interesting life. I had a vested interest in suppressing my identity, my claim to speak about myself, my claim to equality in the relationship. The terms of exchange shifted such that I allowed him to dominate. The effort required reasserting equality of regard, to articulate a rationale for friendship rather than research was too much for me. And, anyway I did not want to make that effort. The change in my relationship with Paul was crucially redefined by my view of his behaviour regarding his marriage break up, rather than the fieldwork. Moreover, the scrutiny of Paul as businessman did contribute to my liking him a little less. More personally, the research relationship between Paul and I exacerbated for me what I saw as his lack of regard for who I was, what I was doing with my life, and perhaps the importance of research compared to the "real world" of business and entrepreneurial wealth creation. Paul had become successful professionally before I had. I had spent many years just being a student in my late 20s and early 30s (I was 34 in 1997 when the research began), whilst he was a serious businessman. Paul was not really interested in what I was doing in my new career as an academic. I could not see how this attitude could be the basis of a good friendship.

Despite these reflections, I still agree with the conclusion I came to in the appendix that the use of friendship in research, and specifically with respect to gaining access to difficult to reach research environments, can be a good thing. Though likely under-reported because of orthodox fears of pollution and bias, in my experience friendship relations are very much part of social science research. Friendship connections between peers are often the starting point of gaining access. The building of friendship relationships in fieldwork is also very common. I mention below for instance, that as a result of my experience at Fenderco I sought to maintain greater distance in subsequent ethnographic fieldwork at a steel works in Australia (2003-2004). Nevertheless, new friendships were formed. One key friendship with Michael Hughes (real name), a supervisory-level manager at the plant, provided some of the most important methodological (Down and Hughes, 2009) and theoretical insights (Down and Reveley, 2009) of that research.

However, as Clifford Geertz has famously written (1968), there is a conceit at the heart of the relationship with respondents. As researchers we have very little to offer:

> […] the only thing we have to give, after bribery and highfaluting idealism is discounted, "is oneself" (Geertz, 1968, p. 151). That is, we tend to build up personal and emotionally engaged relationships in order, in part, to maintain a degree of self-respect. But these relationships are partial fictions and produce an ironic and "inherent moral asymmetry" (1968, p. 151). The partial fictions that both researcher and respondent ascribe to the social interaction are ironic in that although the researcher and his/her subjects go along with the fiction, they are "never completely convincing for any of the participants" (Geertz, 1968, 2006).

Moreover, as Down *et al*. (2006, p. 90) continue, "The inherent moral ambiguity of the researcher (friend, emancipator or judge? Emotionally engaged or detached?) places him or her in a contradictory position".

Thus, it is no wonder that the new identities of researched and researcher for Paul and I created some tensions. And, whilst in footnote 5 (Down, 2006, p. 128, page 12 in this paper) I write that Paul and I got over the initial "embarrassment and amusement at taking on the roles of interviewer and interviewee", I think it is fair to say that looking back now the moral ambiguities of our new relationship may well have been more corrosive than I realised at the time.

It is certainly the case that I have resisted temptations to research other friends who have become successful business owners. But to say, upon reflection, that I would do things differently today is not that illuminating. Now I would do things differently, because I am a different person, with different opportunities and contexts for the research I conduct. But if you were my PhD student or an early career researcher faced with a similar opportunity, I would encourage you to do the same or similar. However, whatever choices are made, beware, researching friends, acquaintances, enemies or anyone with whom you have an existing relationship will undoubtedly change the relationship.

1997 is a long time ago. Much has changed in ethnographic practice. Today I read and see presentations of ethnographies utilising social media as methods, the use of images as data and greater experimentation. Traditional ethnography is still popular. Workplace and organisational ethnography (see Ybema *et al*., 2009) has, despite regular conservative declinist fears of a crisis in quality (see for instance Down, 2012), become a more established part of business and management scholarship, in the sense of being attractive to scholars, journal editors and reviewers, and in having more institutional presence (for instance, the creation of this journal). Reading through *Narrative of Enterprise* today, and the methodological appendix in particular I am struck at how unbounded by time it feels. This is clearly somewhat fortunate for its current purpose, in presenting it to readers of *The Journal of Organizational Ethnography.*

What follows below is how the chapter appeared in the book. The endnotes appeared in the original chapter whereas the footnotes at the bottom of each page have been inserted as and where additional explanation was required.

Methodological appendix

Writing soap operas[1]

Introduction. Why an appendix? What is it about the methods and methodologies used in doing the research at Fenderco[2] that prompts their relegation to an appendix? This appendix serves the purpose of clarification and not justification. It is an appendix because it is of specialist interest. There is a need to show ones hand, not in defence of ethnography and interpretivism as a general mode of inquiry but in regard the more specific choices made.

One of the first among these choices was a general orientation to doing research that saw methods and methodology as means not ends. Which is what they should be. Right from the decision to do the research I wanted it to be as interesting and meaningful as possible. For me this means I am suspicious of those that cede too much importance to the prescriptions and rules of research, over the practical problems the research is hoping to illuminate. In taking this stance I have forever joined in with the unruly scrum of pragmatism against the pack of dogmatists and the "hegemony of the methodologists" (Ackroyd, 1996, p. 449). This appendix shows what this stance has meant in practice.

A second purpose of this chapter is to reaffirm a connection with the experience of doing the research. It has been eight years since I first ventured to Maltonbury as a researcher. In writing this book I have sometimes worried about how my experience has been transformed via my doctoral dissertation, various journal articles and now this book. The story I tell has progressively become a more coherently organised and conceptually orientated one. It seems to have much more of a point to it than it once did, which is clearly a good thing. All forms of narratives need to have the random events of experience filtered and selectively appropriated. There is something lost though in this process of ordering and conceptual articulation: something real and earthy, and for me particularly attractive and meaningful. This appendix gives something more of that original experience back. It provides some of the notes in the margins: “The Making of *Narratives of Enterprise*: the Director’s Cut”.

A third purpose seeks to add to the analysis, albeit in a specialised manner. All research topics have methodological implications. Unlike some topics, however, I work on my self-identities via the same processes that John, Paul, Will and Mark do. We all do. Hence my presence in the research is also an unavoidable object of research. Not in the hackneyed and moribund sense of polluting otherwise pure research, rather, as an opportunity to extend the study. The reader, having read about how I too would construct self-identity in conversing with Paul and the others, or through the writing of this book, will be able to better assess the manner in which the foregoing research proper was crafted.

These choices are subsumed within a more traditional chronology of ethnographic research: starting with research design and access, collecting information through observation and interviewing, problems and ethical issues in the field and finally writing concerns.

Doing the fieldwork. I started this book with a little story of when I used to run a record label. This experience of being an entrepreneur of sorts has proved a key self-defining episode of my life. I guess even my academic interest in enterprise is an aspect of my own quest for self-knowledge. The occasional e-mails I still receive about my record label, often from people who were not even born at the time, prompt me to think about who I was back then. The academic artefacts I produce should be seen as stemming from a fascination with the processes of enterprise. I feel entrepreneurial in what I think and do. Along with Paul and John I construct my academic self-identity in opposition to the routine, conservative and bureaucratic institutions around me.

When it came to doing my Warwick masters dissertation in 1994 (eventually published as Down and Bresnen, 1997), it seemed obvious and inevitable that I should research small firms and entrepreneurs. Fellow masters student Chris Moule’s covert ethnography (1998), my other subsequent research, and studies by Monder Ram (1994) and Ruth Holliday (1995) slowly guided and inspired me towards a desire to undertake further much needed ethnographic research in a small firm for my doctoral dissertation. The problem was how to do the research.

Seeing as I already knew Paul and he had already acted as an interviewee (Down and Caldwell, 1996), it seemed sensible to talk to him about my proposed doctoral research. It took a little time, however, to convince myself that having a research site with a friend in situ might be a legitimate way of researching. Even interpretively empathetic colleagues warned of the potential risks, whereas the literature said that despite this there were many benefits to the admixture of friendship and research in studying smaller firms (Ram, 1994; Hobbs, 1988; Moule, 1998). Kondo (1990) and Holliday (1995) have shown that friendships are often and inevitably formed in long-term qualitative research even when they do not pre-date the research itself. One way of achieving thick description is already in some way being part of what is being described. Encouraged by this literature and my supervisor, I asked Paul and the deal was done: the research was on.

Our relationship was always an easy going one. We had first met in the early 1980s. We were both relatively new to London, Paul beginning his career at Harbourco, and myself playing in and managing bands, and packing meat for Debenhams in Oxford Street. We were comfortable and open with each other but were not close friends, and after a short period of living in opposite apartments in a house in Ladbrooke Grove we would meet for a beer every few months: I a fringe player in his clique and he the same in mine. And, with the occasional break – such as his four-year stay in Asia working for HarbourCorp – this is how our relationship continued until the research started.

My visits to Paul's and other friends in the Maltonbury[3] area were regular even before starting interviewing and taking field-notes in early 1997. I would base myself at Paul's rented "Castle house" and spend the weekend there whilst visiting industrial placement students during the week. This university work continued throughout the research period (the last research-oriented visit took place at the end of 1998).

Upon arriving as a researcher therefore, Paul, John and the others were already an occasionally regular and normal part of my life. However, it came as something of a jolt to actually start researching. Initially I felt authentic neither as a researcher nor as a friend. My field-notes describe the first recorded conversation with Paul as being harder than was expected. They also describe how I was conscious of the potential deleterious effect the whole process might have on our friendship, and how I "tried to start [the research] fairly innocuously, and have skirted some issues. We need to establish our 'working' relationship: the limits, style and scope of the conversations". The awareness, therefore that access is a continually negotiated process (Czarniawska, 1998, p. 33), however, strong the researcher/subject bond, was ever present. My reason for being there (as the "researcher") was something that was continually and implicitly being negotiated.

But there I was. The research was taking place: information was being collected. Two methods were used to collect the words and behaviour used throughout this book: recorded and noted conversations, and noted observations. In addition, of course, the field-notes include my own reflections upon what took place at or near the time.

Observing. Though it was not explicitly discussed in our access "negotiations" the research was planned knowing that it would not involve continuous and lengthy participant or non-participant observation in the workplace. There were two reasons for this understanding. First, as a full-time lecturer with no opportunity for a sabbatical and with little in the way of specific skills to offer Fenderco in exchange for continuous access it was not possible to reside at the firm. Indeed, though I did spend many an hour hanging around in the office, either engaging in small talk, using the office computers, fax, photocopier and so on, or waiting to talk to someone, waiting to drive to a site somewhere, or waiting to go for lunch, actually observing everyday working practices for long enough so that it would produce particularly inspiring material was not feasible. I was not and could not become an insider in the office. Thus, the second reason was as we saw in Chapter 5 Space that in an open plan office with five people hard at work (buying, selling, drawing plans, telephoning, calculating, thinking and talking) there was little room for a fly on the wall.

When first thinking about the research I had thought that not being able to do continuous observation would be a problem. However, the literature soon showed that fieldwork could be far more eclectic and pragmatically assembled. Van Maanen (1988, pp. 127-130, 139) encourages adventurousness and experimentation in the field and suggests that the primacy of "being there" continuously is no guarantee of avoiding the production of formulaic or atheoretical written research (Van Maanen, 1988, pp. 127-130, 139: 12). Observation at the office, on site, at Paul's home or socialising in the pub was nevertheless a key aspect of the research.

However, the conceptual emphasis on what Paul and John say about themselves in interviews and conversation precludes a dominant observational component.

For example, whilst it would have been good to have made more real-time observation of generational encounters[4], this aspect of self-identity is more readily observable in conversation about past events, situations and decisions: it happens naturally in the spoken rationalisations of action (Giddens, 1976, p. 156).

For those that see qualitative research as a mirror of reality, or a window on the truth, the danger that accounts of the past will be coloured by current circumstances is a real concern. Because this research is interested in self-identity this so-called danger is exactly the point: identity is a narrative of the past, and identity formation is an inherently temporal process. Thus it is what they think, rather than know, about the past and how they articulate it that is important.

My observations do serve to confirm what is said, such as when I saw the different moods that would pervade the Fenderco office. More interestingly the observed material would often provide an alternative version to the talked about sense of themselves they present in the interviews, such as the change that Paul and John underwent when going on site.

Neither the talking self nor the observed self holds any privileged representation to a true self. Being present and observing provided me with a way of both building relationships and as a way of checking my understanding of what was going on. Thus, whilst I did not write down or capture all the words and conversations seen and heard in the office, of an evening over dinner at Paul's home, or in the pub, my natural social sensibilities and the "mutual knowledge" (Giddens, 1976, p. 161) I share with them must have mixed and cross-referenced with my reflections about the research, checking whether my impressions from inside the research were consistent with those from outside.

Interviewing. The recorded interviews with Paul, John, Will and Mark took place in a variety of settings: Paul's home, Mark's home in an extended lunch break, in John's office and so on. Though access was agreed actually pinning people down proved harder in practice. Whilst staying in Maltonbury I would often walk to the Fenderco office and find that the intended interviewee was not there or was busy. I therefore had to be fairly flexible and "entrepreneurial" (Ram, 2000, p. 657; Hobbs, 1988, p. 7) and take my opportunities as they arose.

The conversations were open and wide-ranging. There was no specific structure to the conversations beyond those culturally ascribed conventions common to all social interaction and a guiding sense of what is and is not interesting. But, as Burgess has noted this does not mean that they were conversation without purpose (Burgess, 1982, p. 107). The conversations with the owner-managers were generally structured in terms of a chronology of their relationship to each other and the history of their careers. The conversations with Will and Mark were explicitly focused towards their understandings of, and relationships with, the owner-managers.

The process of spending time and sitting down with individuals had some interesting effects. The interview changed relationships. Will's attitude to me changed considerably after our interview. In The Grinning Cat pub later that week he now spoke to me about his upbringing and personal trials and tribulations. As I stated in my field-notes: "It was as though the interview had been a confession, and now we have a bond". The process of sitting down, one-to-one, and doing the interview seemed to draw myself closer to all.

Despite the importance of interview material in this study, like all methods it is not unproblematic. Paradoxically, though interviewing is now widely accepted even by those philosophically unsympathetic to qualitative methodologies (hence the rise of the quantitative frequency-based approach to interview data), recently a critique of the interview has emerged (Alvesson and Kärreman, 2000; Atkinson and Silverman, 1997). The automatic and assumed authenticity of the interview is now questioned: as Burgess put it, “how do we know the informant is telling the truth?” (Burgess, 1982, p. 109).

Happily, the interview data in this book is not being presented as truth (except of course in that it is not made-up! – on the validity of even this, see Watson, 2000). What is represented by the interview materials are self-identity narratives that Paul, John and myself have jointly produced. Atkinson and Silverman argue that the form of the interview is embedded so deeply into our society that “the self is rehearsed” for the interview (Atkinson and Silverman, 1997, p. 314). The initial difficulties I experienced in interviewing Paul were, I think, partly due to the mutual sense of embarrassment and amusement at taking on the roles of interviewer and interviewee[5]. The interviews were part of how Paul, the others and myself produced self-identity: less a method for ascertaining the truth than a vehicle for producing it.

This can be seen in more detail in an excerpt from a conversation with Paul. It was around 10 p.m. and the interview had finished. We were about to pop down the pub for last orders[6] and our conversation turned towards our mutual experience of being entrepreneurs: the tape recorder was turned on again and Paul said:

> There are very few people that I have worked with and for that are prepared to take that risk to run their own business. And that’s what sets entrepreneurs above employees (most of them), for most […] for many people their personal objective is the security of having a job and not wanting to put anything on the line […] [pause].
>
> […] the responsibility and effort […] [pause]. As you know, despite being really far too young and not having any business sense or anything [when I ran my record company], I do know basically, more or less what it’s like [to be an entrepreneur] and I suspect that in the future I may well. […] [pause]. I don’t know if I have ever told you that I was chatting to my boss after the end of the first year [of working in academia] and I asked her “what do you envisage me doing in the future?” and she said she “could see me running my own research company”, you know […] I hate being an employee.
>
> It’s [being an entrepreneur] obviously within you. You have done it before and it’s part of your psyche, it’s there and it may come back one day.

In this slice of conversation we are both constructing and identifying our biographical narratives with the mutually desired character of the entrepreneur: we are producing this particular sense of ourselves.

This conversation raises interesting questions about the identity self-management of the researcher (Czarniawska, 1998). Was I constructing a “true” sense of myself when talking of possibly working as an entrepreneur in the future? To what extent was I managing or performing my identity or my research? I honestly do not know. It is no truer about my own self-identity than a conversation I might have now with an academic colleague about being a particular type of academic. Neither my authorial voice nor Paul and John’s voices have any inherent authenticity. The voices do tell us something, but they need to be pragmatically accepted, not seen as truth.

Friendship, research and self-identity. All research where people are the subjects involves the investigator adopting certain roles, behaviours and projecting certain identities. Hobbs, for example, writes of managing his image through the clothes he wore being an important aspect of his fieldwork. He also wrote that "if life was entrepreneurial and sharp, then so would I be. Sexist and chauvinistic, no problem. Racist? Well, no" (Hobbs, 1988, p. 11). Similarly, I also had choices to make about how to be: what identity to adopt. Throughout the period of the research I felt that the very nature of talking to and observing people with a purpose in mind, and thinking academically about what was done and said (i.e. being a researcher), somehow involved me being inauthentic; a covert participant. I do not mean that I did not talk about my interests with my informants, or that I misled them. But this self-conscious feeling was nevertheless pervasive. An example of this reflexive tension in conducting research can be seen below in the dialectic between my identities as friend and researcher.

Mid-way during the research Paul began experiencing difficulties in his marriage. I was involved with this both as a friend of his wife and more specifically as a friend and confidant of Paul. These difficulties also had their effect on the fieldwork. One visit to Maltonbury for instance, when he was particularly preoccupied with work and these difficulties, "produced no talk with Paul" (from field-notes, meaning an extended recorded or noted conversation). On one particular evening in The Grinning Cat the tensions between being friends with both Paul and his wife overflowed into a frank exchange of views and a disagreement about the nature and scope of our friendship and the level of involvement in each other's affairs. The issue of the research was not raised in the conversation. Though not especially dramatic in itself, this event did mark something of a significant change in the nature of our relationship. I was aware, as this extract from my notes show, of the impact these tensions were having on the conduct of the research:

The whole thing [Paul's marriage problems and their effect on our friendship] is a problem with regards the research as I am in a sense too close to the subject. Now, whilst this presents advantages, the whole project is subject to the ebbs and flows of my relationship with Paul. […]. The point is that maybe we are too close? […]. There are some things which impinge upon the research which are not necessarily the proper subject of inquiry. Can the research carry on under these circumstances? Where do I draw the line?

Like Hobbs, who drew a line between being sexist and being racist in the entrepreneurial pursuit of his research I too drew a line between being a friend and being a researcher in this particular incident. For most of the time the edge between these identities was happily indistinct and comfortably accommodated in the confusion and intimacies of everyday life, but for me on this particular evening the boundary was clearly delineated. These tensions between the identities of researcher and friend are a problem of the tension between my sense of who and how to be. The me that is a friend was ultimately in this particular instance more important to me than the me at work. The research did carry on, productively and for some time, though ultimately this change in our relationship meant that the fieldwork was cut short.

These issues remain unresolved for me both as researcher and friend. When I next went into the field – at the coke ovens of a steel works in Wollongong, Australia (Down, Badham and Garrety, 2003) – I sought to maintain more of a distance. The "emotional investment" (Hobbs, 1988, p. 10) that is made in research raises difficult personal, professional and epistemological issues. From a personal point of view I would rather not have to deal with these issues again so profoundly again. The issues of my identities – as "entrepreneur", "friend" and "researcher" – have featured occasionally in the story of Fenderco, but the topic of this book is the processes of self-identity construction and maintenance of two entrepreneurs. My relative absence from the story is also because there are some legitimate limits to what can be told.

Limits to the telling. Thus, whilst my reflexivity is important as it recognises that I am part of what is being studied, this does not mean that I should tell the reader everything about my life. This is a confessional ethnography about self-identity in a small organisation setting not "a black hole of introspection" (Van Maanen, 1988, 92). This book is framed by a concern with organisational issues (small firms, entrepreneurial identity, work, etc.) and is therefore is inherently limited.

However, being part of what is being studied means that other aspects of my informant's lives were seen, heard and noted. But the details of Paul's marital relationship cannot constitute part of the analysis. The main reasons for this are because it is not as relevant as what was considered, selected and presented (Becker, 1986). Importantly, it would also not be decent to do so. Some boundaries are placed around our experience as researchers. An aspect of these boundaries includes my own emotional investment and sense of loyalty to the people of this study.

There is another limit to the telling. There is, as Philip Roth recognises[7], in specific relation to self-identity, also the limit to what one can possibly know about other people. At one point in the fieldwork for instance, after his marital difficulties became apparent, Paul suggested to me that he might "break with Fenderco in the not too distant future". This comment came as something of a surprise given the way Paul usually talked of the company's continuing success and grand future, and it raised paranoid doubts in my mind about what I really knew about what was going on, as well as the obligatory selfish fears about continuing access! At the time in my field-notes I wrote:

> He seems to be suggesting that he will break with Fenderco in the not too distant future. Might it be that there are more substantial problems existing in the company? How do I *know* what is going on? What do I consider legitimate and illegitimate information? Questions, questions […].

In this sense the data I have collected, in terms of what it or any data can tell about self-identity, reminds me of what solicitors have to contend with in constructing their narratives about past events. Court evidence is never complete, and can ignore facts deemed illegitimate under certain legal criteria. Different interpretations and theories are also made using the same facts. Qualitative research materials, however, exhaustive or comprehensive are the same: there is always something that is left out. Ultimately, it depends on the story one wishes to tell and who your audience is.

Ending the research?. The last research visit to Maltonbury took place in November 1998. It was then that I realised that I was not willing to continue and complete the planned research. The original research design had ambitiously planned for three stages, which in addition to punctuated interviewing and observation of the owner-managers and employees (stage one and two), would include interviews with the "wider personal, social and commercial networks" (Down, 1999a, p. 277) of the owner-managers.

My field-notes from this visit explain that the combination of Paul's continuing marriage difficulties and the tensions in our friendship meant that "I have lost the enthusiasm [for the research], and [thus also] in a sense the 'access': this in the sense that I am not really part of the organisation now". Nothing, however, particularly dramatic had occurred. There had been no censure or closing of the gates. Paul had even agreed that I could make appointments to see his solicitor and accountant as a first foray into stage three of the research. Subsequent discussions with Paul sometime after the research had finished suggested that though he was aware of the change in my attitude to him, he was generally unaware as to what had caused it. But I had had enough. I was also sufficiently excited about the data that had already been collected to think that I had enough to be going on with.

In some limited sense though, the fieldwork did not stop at all. My relationship with Paul and the others had changed and I was not engaged with these people as a researcher any longer. Paul and I were still friends (though the relationship had changed). I still visited Maltonbury. I still talked with them about Fenderco and my research, but I had stopped being the "researcher", taking notes and looking for meaning (well as much as I could – I may have stopped taking notes, but it is a very different matter to stop thinking, especially when Fenderco continued to provide the main context of my research work). I was just me again: the friend, acquaintance and one of those "fucking academics who don't know anything about the real world", to quote Will. Despite my enthusiasm for the concepts I was developing from my data analysis, disappointment hung over the fieldwork like one of those personalised cartoon rain clouds, following me around. In a sense my wiser, older, more conservative colleagues had been right about the risks of combining friendship and fieldwork. As life, teaching, conference papers (Down and Sadler-Smith, 1999), and other research work (Down, 1999b) trundled on, and as the fieldwork experience slipped away into distance memory, I was able to feel more confidence about what I had achieved: what I found in the data inspired, fascinated and importantly kept me working.

Research as writing and constructing identity. And work for academics means writing. It is appropriate therefore to conclude with some reflections on that process. What has impressed me writing all these years about Fenderco is how important a vehicle it has become in constructing my own sense of self. The reading, writing and referencing conventions of academia do more than provide a common base on which to create knowledge or facilitate scholarly conversations (Czarniawska, 1998). They are also the equipment of professional socialisation: they form our rules of expression and a guide to appropriate behaviour. The influences on my writing style and the citations I use are the locating evidence of that socialisation. Writing is important and the story I tell is ultimately a piece of writing more than it is a piece of fieldwork. I have always felt more craft writer than scientist. The point is to be aware of the way I have chosen words, headings, citations, analogies and so on.

The craft of academic writing also forms a narrative environment for the production of my own career identity. That is, what type of academic I want to and can be, and the meaning this has for my own narrative of self. Just as Paul and John talk the talk of the entrepreneurial clichéd narrative in constructing their identities, I write the text of a certain type of academic. The crafting of this book and my own career more generally has created a space in which I have consolidated the disparate elements of events of my own biography into a secure and effective self-identity. In particular I have sort to consolidate the entrepreneurial together with the academic narratives. Though my activities as independent record label owner, meatpacker, London underground worker, student and academic might seem fragmented, the narrative which I have constructed, am constructing here, is of a piece. Even my academic interests in small firms, work and organisations are the consequence of the narrative crafted from the events of my life.

I could go on. I did not intend this work to have such personal meaning. Untangling the narrative threads that make up our own lives is arguably harder than looking at others. Thankfully my task has been to tell a story about others, I hope it has been an interesting one.

Notes

One of my supervisors, Eugene Sadler-Smith, asked me early in the fieldwork, when I would return to my office enthusing about my experiences: "Was your aim to write a research project or a soap opera?" (Down and Sadler-Smith, 1999, p. 15). For some time neither of us was too sure about the answer.

This was the name given to the small business which designs and sells "fendering equipment: large steel and rubber structures that are designed to stop ship hulls and wharf sides from being damaged in berthing and manoeuvring procedures" (Down, 2006, p. 3).

The fictitious middle England town where the research took place.

Chapter 4 "Generations". See also Down and Reveley (2004).

Eventually, however, we seemed to incorporate this new dialogue into the modus operandi of our relationship. The amusement and embarrassment soon evaporated from our conversation, replaced for me by a workaday sense of fieldwork being achieved, and for Paul a sense of an opportunity to talk about and reflect upon his project of Fenderco and his life more generally. Undoubtedly this new context for our relationship changed its nature, especially in the extent to which Paul's career activity became the predominant topic of conversation. But the mutual exploration of our careers had always provided a central focus of our friendship anyway.

In UK pubs the bar staff will shout "last orders at the bar please" ten minutes before closing the bar. Most pubs allow 20 minutes or so for drinking-up before closing.

This is a reference to a quote from Philip Roth's (1998, p. 35) *American Pastoral* used in Down (2006, p. 105) which reflects on the illusion and impossibility of knowing the "interior workings and invisible aims" of other people.

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